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Published 07/12/89

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INTERIM REPORT OF THE COLD FUSION PANEL TO THE ENERGY RESEARCH ADVISORY BOARD

THIS IS A PRELIMINARY DRAFT THAT HAS NOT YET BEEN SEEN BY ALL PANEL MEMBERS. IT WILL BE SENT TO ALL PANEL MEMBERS FOR THEIR COMMENTS. AFTER THEIR COMMENTS ARE INCORPORATED, THE PANEL'S REPORT WILL BE SUBMITTED TO THE FULL ERAB. THE FULL ERAB WILL REVIEW THE PANEL'S REPORT FOR POSSIBLE MODIFICATION OR REVISION AND APPROVAL PRIOR TO SUBMISSION TO THE SECRETARY.

INTRODUCTION

As a result of the startling announcements in March 1989 by Utah scientists claiming the attainment of cold fusion, the Secretary of Energy requested (see Appendix A) that the Energy Research Advisory Board (ERAB) convene a panel (see Appendix B) to assess the possibility of cold fusion. The panel meetings and schedule of laboratory visits are summarized in Appendix C.

Since the above announcement, many laboratories worldwide have initiated research in cold fusion. In the United States, a major effort has been undertaken to search for cold fusion by a large number of research groups at industry, university, and national laboratories. Unfortunately, at the present time, the reports from different laboratories are quite divergent. Some laboratories claim excess power production attributed to cold fusion, usually for intermittent periods and for various periods of time but with no supporting evidence for the production of commensurate quantities of fusion products. Other laboratories find no measurable excess power production and no measurable high levels of fusion products. Some laboratories attribute the discrepancies to inaccuracies in measurements, others to non-reproducibility of a new and not understood process. Tritium levels above normal have been reported in some cells following electrolysis but not in others. Neutrons near background have been reported in some D_2O electrolysis and pressurized D_2 gas experiments, but at levels 10^{12} below the amounts required to explain the experiments claiming excess power.

In the past 8 weeks the Panel or subgroups thereof have participated in the Workshop on Cold Fusion in Santa Fe, have visited the laboratories listed in Appendix C, have studied the open literature and numerous privately distributed reports, and have participated in many discussions.

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GENERAL CONCLUSIONS

Although the Panel's task is not yet completed, the Panel finds that the experiments reported to date do not present convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion. Indeed, evidence for the discovery of a new nuclear process termed cold fusion is not persuasive. Hence, no special programs to establish cold fusion research centers or to support new efforts to find cold fusion are justified at the present time.

However, there remain unresolved issues and scientifically interesting questions stemming from reported cold fusion efforts. Some of these are relevant to the mission of DOE and should be handled by carefully focused and cooperative efforts within current programs by normal mechanisms for project selection.

The reports of excess heat and fusion products are assessed in separate sections. Preliminary recommendations are summarized in the final section.

CALORIMETRY AND EXCESS HEAT

The claim for electrochemically charged palladium cells as prospective energy sources rests on reports of "excess heat" (or, more precisely, excess power) that cannot be accounted for in the thermal balance normally applied to water electrolysis. Among the issues the Panel addressed in site visits were whether the power levels themselves are being accurately measured and whether the reactions being considered in these cells are, in fact, satisfying the chemical assumptions made. These heat measurements have been done with calorimetry varied as to technique and to levels of precision and accuracy. In most cases, calorimetric effects attributable to excess heat are very small and the calorimetric measurements are difficult and subject to subtle errors arising from various experimental problems.

For the purposes of this report, the calorimetry is usefully differentiated as to whether the D_2 and O_2 gases are allowed to exit the cell completely unreacted or are intentionally catalytically recombined to regenerate D_2O and to recover the corresponding heat. In the case of open cells, where the gases are assumed to be vented without reaction, any output power (as heat) greater than the electrical input power minus the power equivalent of the D_2O formation enthalpy [1.527 V x I (cell current)] is considered excess, a result reported by several groups. In closed cells with total recombination (and with a deuterium-charged Pd electrode), the total electrical power in and total heat power out would normally balance (as for Pt and Pd electrodes in light water). At present no experimenters who have performed calorimetry with closed cells under strict recombination conditions have reported any excess heat. Another important point is that most of the reported measurements with open cells are actually power measurements, and the data have not conclusively demonstrated that the total amount of energy produced (as heat and chemical energy) exceeds the total electrical energy input.

Since the claimed excess heats have, in most cases, been of a magnitude significantly less than the 1.527 V x I factor itself, issues of calibration, reliability, and support of the assumptions of zero recombination are especially critical. The Panel's site visits have identified experimental uncertainties, e.g., nonlinearities of the calibration in power output vs. temperature, time dependence of calibration, and doubtful accuracy of data acquisition relative to the magnitude of the effects asserted. Even in laboratories that report excess heat, this effect, under apparently identical conditions, is often not reproducible. In none of our visits to the different sites did we see an operating cell that was actually producing excess heat. So far, we have seen no experimental results that are sufficiently free of ambiguities and calibration problems to make us confident that the steady production of excess heat has been observed. However, there are reports of sporadic temperature "excursions" or "bursts" that apparently represent power outputs significantly larger than the input power. These events cannot be attributed to problems with accuracy or calibration alone and are presently not understood. In general, the calorimetry to date does not persuasively demonstrate the production of excess heat, but the bursts will require evaluation in the Panel's final report.

FUSION PRODUCTS

Since deuterium fusion necessarily yields fusion products (neutrons, protons, tritium, ^{3}He, ^{4}He, gamma rays), it is essential to establish the presence of such products in any claim of fusion. Each watt of power must be accompanied qualitatively by 10^{12} particles per second. This makes product detection by far the most sensitive method to search for fusion. Results to date on fusion products are summarized in the following paragraphs.

Neutrons are an established signature of the well studied d+d fusion reaction. Although many experimenters report no neutrons, some report as many as 1 neutron per second. If confirmed, this rate would be of some scientific interest (even if not indicative of cold fusion). This rate is so far below the 10^{12} neutrons per second required for 1 watt that it is of no interest as a practical energy source.

Numerous experimenters have sought tritium production in electrochemical cells and have found no excess tritium. One group reports finding up to 10^{14} tritium atoms (neglecting losses to the gas phase) in each of several cells with Pd cathodes and Ni anodes. Some of these experimenters report neutrons produced from similar electrochemical cells, but at a rate of about one neutron per second. If the tritium were a result of deuterium fusion, the rate of neutron production should be comparable and thus some 10^{10} times greater than reported.

Another important fusion signature is ^{3}He which should be detectable within a cathode after operated at fusion power levels of watts. It has been postulated that the cold fusion reaction might conceivably proceed predominately by the production of ^{4}He and thermal energy. None of the researchers to date, including those reporting the production of heat, have reported ^{3}He or ^{4}He above the detectable level of 10^{9} atoms. One watt-hour of energy corresponds to more than 10^{15} atoms.

Low level cold fusion in geologic processes has been proposed to cause high ^{3}He/^{4}He ratios and tritium abundances associated with volcanoes. Several laboratories are currently attempting to detect volcanic tritium.

INTERIM RECOMMENDATIONS

1. The Panel recommends that the cold fusion research efforts in the area of heat production focus primarily on confirming or disproving reports of excess heat. Emphasis should be placed on calorimetry with closed systems and total gas recombination, use of alternative calorimetric methods, reasonably well characterized materials, exchange of "promising" electrodes between groups, and careful estimation of systematic and random errors. Cooperative experiments are encouraged to resolve some of the claims and counterclaims in calorimetry. Such experiments should be pursued at a limited number of laboratories and supported at a modest level on the basis of competitive proposals. At the present time, the panel recommends against any significant expenditures to establish cold fusion research centers or to support new efforts to find cold fusion.

2. A shortcoming of most experiments reporting excess heat is that they are not accompanied in the same cell by simultaneous monitoring for equivalent fusion products. If the excess heat is to be attributed to fusion, such a claim should be supported by measurements of fusion products at commensurate levels.

3. Experiments designed to check the reported production of excess tritium in electrolytic cells are desirable.

4. Experiments reporting fusion products (e.g., neutrons) at a very low level, if confirmed, are of scientific interest but have no apparent applications to the production of useful energy. Continued support of such experiments at modest levels is justified, provided the proposals for such research are evaluated in comparison with other DOE research proposals. In view of the difficulty of these experiments, collaborative efforts are encouraged to maximize the detection efficiencies and to minimize the background.

06/20/89 From Dick Garwin ~~a propos~~ re:
Cold Fusion

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CLASS 1

04/00/68 "Pathological Science," by I. Langmuir. (040068..IL)

PATHOLOGICAL SCIENCE by I. Langmuir

Transcribed and Edited by R. N. Hall

Report No. 68-C-035

April 1968

GENERAL ELECTRIC
RESEARCH AND DEVELOPMENT CENTER
SCHENECTADY, NEW YORK

TECHNICAL INFORMATION SERIES

AUTHOR Transcribed and Edited by Hall, RN

SUBJECT scientific method; psychological influence; threshold phenomena

NO. 68-C-035

DATE April 1968

TITLE Pathological Science by I. Langmuir

G.E. CLASS 1

GOVT. CLASS -

NO. PAGES 13

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SUMMARY This report is a transcript of the talk on "Pathological Science" given by Irving Langmuir at the Research Laboratory on December 18, 1953. It deals with what Langmuir called "the science of things that aren't so," and in it he gave a colorful account of several examples of a particular kind of pitfall into which scientists may sometimes stumble.

KEY WORDS Allison effect, ESP, Rhine, flying saucers, N-rays

INFORMATION PREPARED FOR:

PATHOLOGICAL SCIENCE

I. Langmuir

(Colloquium at The Knolls Research Laboratory, December 18, 1953)

Transcribed and edited by R. N. Hall

PREFACE

On December 18, 1953, Dr. Irving Langmuir gave a colloquium at the Research Laboratory that will long be remembered by those in his audience. The talk was concerned with what Langmuir called "the science of things that aren't so," and in it he gave a colorful account of several examples of a particular kind of pitfall into which scientists may sometimes stumble.

Langmuir never published his investigations into the subject of Pathological Science. A tape recording was made of his speech, but this has been lost or erased. Recently, however, a microgroove disk transcription that was made from this tape was found among the Langmuir papers in the Library of Congress. This disk recording is of poor quality, but most of what he said can be understood with a little practice, and it constitutes the text of this report.

A small amount of editing was felt to be desirable. Some abortive or repetitious sentences were eliminated. Figures from corresponding publications were used to represent his blackboard sketches, and some references were added for the benefit of anyone wishing to undertake a further investigation of this subject. The disk recording has been transcribed back onto tape, and a copy is on file in the Whitney Library.

Gratitude is hereby expressed to the staff of the Manuscript Division of the Library of Congress for their cooperation in lending us the disk recording so we could obtain the best possible copy of the Langmuir speech, and for providing access to other related Langmuir papers.

COLLOQUIUM ON PATHOLOGICAL SCIENCE,
by Irving Langmuir

This is recorded by Irving Langmuir on March 8, 1954. It is transcribed from a tape recording, section number three, of the lecture on "Pathological Science" that I gave on December 18, 1953.

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Davis-Barnes Effect

The thing started in this way. On April the 23rd, 1929, Professor Bergen Davis from Columbia University came up and gave a colloquium in this Laboratory, in the old building, and it was very interesting. He told Dr. Whitney, and myself, and a few others something about what he was going to talk about beforehand and he was very enthusiastic about it and he got us interested in it, and well, I'll show you right on this diagram what kind of thing happened (Fig. 1).

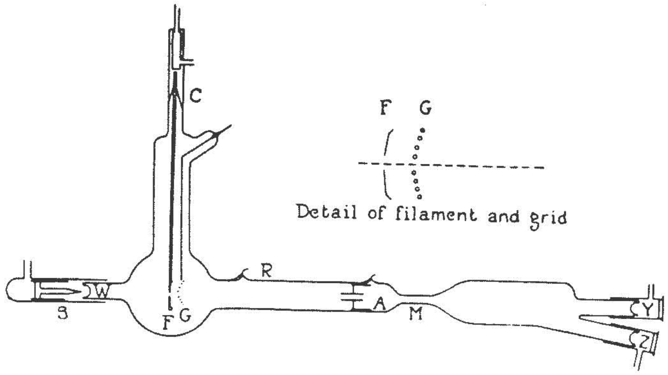

Fig. 1 Diagram of first experimental tube. S, radioactive source; W, thin glass window; F, filament; G, grid; R, lead to silvered surface; A, second anode; M, magnetic field; C, copper seals; Y, and Z, zinc sulfide screens.

He produced a beam of alpha rays from polonium in a vacuum tube. He had a parabolic hot cathode electron emitter with a hole in the middle, and the alpha rays came through it and could be counted by scintillations on a zinc sulfide screen with a microscope over here (Y and Z). The electrons were focused on this plate, so that for a distance there was a stream of electrons moving along with the alpha particles. Now you could accelerate the electrons and get them up to the velocity of the alpha particles. To get an electron to move with that velocity takes about 590 volts; so if you put 590 volts here, accelerating the electrons, the electrons would travel along with the alpha particles and the idea of the experiment was that if they moved along together at the same velocity they might recombine so that the alpha particle would lose one of its charges, would pick up an electron, so that instead of being a helium atom with two positive charges it would only have one charge. Well, if an alpha particle with a double charge had one electron, it's like the Bohr theory of the hydrogen atom, and you know its energy levels. It's just like a hydrogen atom, with a Balmer series, and you can calculate the energy necessary to knock off this electron and so on.

Well, what they found, Davis and Barnes, was that if this velocity was made to be the same as that of the alpha particle there was a loss in the number of deflected particles. If there were no electrons, for example, and no magnetic field, all the alpha particles would be collected over here (Y) and they had something of the order of 50 per minute which they counted over here. Now if you put on a magnetic field you could deflect the alpha particles so they go down here (Z). But if they picked up an electron then they would only have half the charge and therefore they would only be deflected half as much and they would not strike the screen.

Now the results that they got, or said they got at that time, were very extraordinary. They found that not only did these electrons combine with the alpha particles when the electron velocity was 590 volts, but also at a series of discrete differences of voltage. When the velocity of the electrons was less or more than that velocity by perfectly discrete amounts, then they could also combine. All the results seemed to show that about 80% of them combined. In other words, there was about an 80% change in the current when the conditions were right. Then they found that the velocity differences had to be exactly the velocities that you can calculate from the Bohr theory. In other words, if the electron coming along here happened to be going with a velocity equal to the velocity that it would have if it was in a Bohr orbit, then it will be captured.

Of course, that makes a difficulty right away because in the Bohr theory when there is an electron coming in from infinity it has to give up half its energy to settle into the Bohr orbit. Since it must conserve energy, it has to radiate out, and it radiates out an amount equal to the energy that it has left in the orbit. So, if the electron comes in with an amount of energy equal to the amount you are going to end up with, then you have to radiate an amount of energy equal to twice that, which nobody had any evidence for. So there was a little difficulty which never was quite resolved although there were two or three people including some in Germany who worked up theories to account for how that might be. Sommerfeld, for example, in Germany. He worked up a theory to account for how the electron could be captured if it had a velocity equal to what it was going to have after it settled down into the orbit.

Well, there were these discrete peaks, each one corresponding to one of the energy levels in the Bohr theory of the helium atom, and nothing else. Those were the only things they recorded. So you had these discrete peaks. Well, how wide were they? Well, they were one hundredth of a volt wide. In other words, you had to have 590 volts. That would give you equal velocities but there were other peaks, and I think the next velocity would be about 325.1 volts. If you had that voltage, then you got beautiful capture. If you didn't, if you changed it by one hundredth of a volt--nothing. It would go right from 80% down to nothing. It was sharp. They were only able to measure to a hundredth of a volt so it was an all-or-none effect. Well, besides this peak at this point, there were ten or twelve different lines in the Balmer series, all of which could be detected, and all of which had an 80% efficiency. (See Fig. 2.) They almost completely captured all the electrons when you got exactly on the peak.

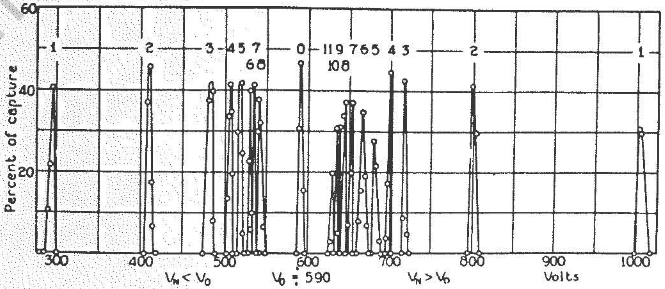

Fig. 2 Electron capture as a function of accelerating voltage. [Copy from Barnes, Phys. Rev., 35, 217 (1930).]

Well, in the discussion, we questioned how, experimentally, you could examine the whole spectrum; because each count, you see, takes a long time. There was a long series of alpha particle counts, that took two minutes at a time, and you had to do it ten or fifteen times and you had to adjust the voltage to a hundredth of a volt. If you have to go through steps of a hundredth of a volt each and to cover all the range from 330 up to 900 volts, you'd have quite a job. (Laughter) Well, they said that they didn't do it quite that way. They had found by some preliminary work that they did check with the Bohr orbit velocities so they knew where to look for them. They found them sometimes not exactly where they expected them but they explored around in that neighborhood and the result was that they got them with extraordinary precision. So high, in fact, that they were sure they'd be able to check the Rydberg constant more accurately than it can be done by studying the hydrogen spectrum, which is something like one in 10^8. At any rate, they had no inhibitions at all as to the accuracy which could be obtained by this method especially since they were measuring these voltages within a hundredth of a volt.

Anybody who looks at the setup would be a little doubtful about whether the electrons had velocities that were fixed and definite within 1/100 of a volt because this is not exactly a homogeneous field. The distance was only about 5 mm in which they were moving along together.

Well, in his talk, a few other things came out that were very interesting. One was that the percentage of capture was always around 80%. The curves would come along like this as a function of voltage (Fig. 2). The curve would come along at about 80% and there would be a sharp peak up here and another sharp peak here and, well, all the peaks were about the same height.

Well, we asked, how did this depend upon current density? "That's very interesting," he said, "It doesn't depend at all upon current density."

We asked, "How much could you change the temperature of the cathode here?"

"Well," he said, "that's the queer thing about it. You can change it all the way down to room temperature." (Laughter)

"Well," I said, "then you wouldn't have any electrons."

"Oh, yes," he said, "if you check the Richardson equation and calculate, you'll find that you get electrons even at room temperature and those are the ones that are captured."

"Well," I said, "there wouldn't be enough to combine with all the alpha particles and, besides that, the alpha particles are only there for a short time as they pass through and the electrons are a long way apart at such low current densities, at 10^{-20} amperes or so." (Laughter)

He said, "That seemed like quite a great difficulty. But," he said, "you see it isn't so bad because we now know that the electrons are waves. So the electron doesn't have to be there at all in order to combine with something. Only the waves have to be there and they can be of low intensity and the quantum theory causes all the electrons to pile in at just the right place where they are needed." So he saw no difficulty. And so it went.

Well, Dr. Whitney likes the experimental method, and these were experiments, very careful experiments, described in great detail, and the results seemed to be very interesting from a theoretical point of view. So Dr. Whitney suggested that he would like to see these experiments repeated with a geiger counter instead of counting scintillations, and C. W. Hewlett, who was here working on geiger counters, had a setup and it was proposed that we would give him one of these, maybe at a cost of several thousand dollars or so for the whole equipment, so that he could get better data. But I was a little more cautious. I said to Dr. Whitney that before we actually give it to him and just turn it over to him, it would be well to go down and take a look at these experiments and see what they really mean. Well, Hewlett was very much interested and I was interested so only about two days later, after this colloquium, we went down to New York. We went to Davis's Laboratory at Columbia University, and we found that they were very glad to see us, very proud to show us all their results, so we started in early in the morning.

We sat in the dark room for half an hour to get our eyes adapted to the darkness so that we could count scintillations. I said, first I would like to see these scintillations with the field on and with the field off. So I looked in and I counted about 50 or 60. Hewlett counted 70, and I counted somewhat lower. On the other hand, we both agreed substantially. What we found was this. These scintillations were quite bright with your eyes adapted, and there was no trouble at all about counting them, when these alpha particles struck the screen. They came along at a rate of about 1 per second. When you put on a magnetic field and deflected them out, the count came down to about 17, which was a pretty high percentage, about 25% background. Barnes was sitting with us, and he said that's probably radioactive contamination of the screen. Then, Barnes counted and he got 230 on the first count and about 200 on the next, and when he put on the field it went down to about 25. Well, Hewlett and I didn't know what that meant but we couldn't see 230. Later, we understood the reason.

I had seen, and we discussed a little at that point, that the eyepiece was such that as you looked through, you got some flashes of light which I took to be flashes that were just outside the field of view that would give a diffuse glow that would be perceptible. And you could count them as events. They clearly were not particles that struck the screen where you saw it, but nevertheless, they seemed to give a diffuse glow and they came at discrete intervals and you could count those if you wanted. Well, Hewlett counted those too and I didn't. That accounted for some difference. Well, we didn't bother to check into this, and we went on.

Well, I don't want to spend too much time on this experiment. I have a 22-page letter that I wrote about these things and I have a lot of notes. The gist of it was this. There was a long table at which Barnes was sitting, and he had another table over here where he had an assistant of his named Hull who sat here looking at a big scale voltmeter. or potentiometer really, but it had a scale that went from one to a thousand volts and on that scale that went from one to a thousand, he read hundredths of a volt. (Laughter) He thought he might be able to do a little better than that. At any rate, you could interpolate and put down figures, you know. Now the room was dark except for a little light here on which you could read the scale on that meter. And it was dark except for the dial of a clock and he counted scintillations for two minutes.

He said he always counted for two minutes. Actually, I had a stop watch and I checked him up. They sometimes were as low as one minute and ten seconds and sometimes one minute and fifty-five seconds but he counted them all as two minutes, and yet the results were of high accuracy!

Well, we made various suggestions. One was to turn off the voltage entirely. Well, then Barnes got some low values around 20 or 30, or sometimes as high as 50. Then to get the conditions on a peak he adjusted the voltage to two hundred and --, well some of those readings are interesting; 325. 01. That's the figure I put down, and there he got only a reading of 52, whereas before when he was on the peak, he got about 230. He didn't like that very much so he tried changing this to . 02; a change of one hundredth of a volt. And there he got 48. Then he went in between. (Laughter) They fell off, you see, so he tried 325. 015 and then he got 107. So that was a peak.

Well, a little later, I whispered to Hull who was over here adjusting the voltage, holding it constant, I suggested to him to make it one tenth of a volt different. Barnes didn't know this and he got 96. Well, when I suggested this change to Hull, you could see immediately that he was amazed. He said, "Why, that's too big a change. That will put it way off the peak." That was almost one tenth of a volt, you see. Later I suggested taking a whole volt. (Laughter)

Then we had lunch. We sat for half an hour in the dark room so as not to spoil our eyes and then we had some readings at zero volts and then we went back to 325. 03. We changed by one hundredth of a volt and there he got 110. And now he got two or three readings at 110.

And then I played a dirty trick. I wrote out on a card of paper 10 different sequences of V and 0. I meant to put on a certain voltage and then take it off again. Later I realized that that wasn't quite right because when Hull took off the voltage, he sat back in his chair--there was nothing to regulate at zero, so he didn't. Well, of course, Barnes saw him whenever he sat back in his chair. Although, the light wasn't very bright, he could see whether he was sitting back in his chair or not so he knew the voltage wasn't on and the result was that he got a corresponding result. So later I whispered, "Don't let him know that you're not reading," and I asked him to change the voltage from 325 down to 320 so he'd have something to regulate and I said, "Regulate it just as carefully as if you were sitting on a peak." So he played the part from that time on, and from that time on Barnes' readings had nothing whatever to do with the voltages that were applied. Whether the voltage was at one value or another didn't make the slightest difference. After that he took twelve readings, of which about half of them were right and the other half were wrong, which was about what you would expect out of two sets of values.

I said, "You're through. You're not measuring anything at all. You never have measured anything at all."

"Well," he said, "the tube was gassy. (Laughter) The temperature has changed and therefore the nickel plates must have deformed themselves so that the electrodes are no longer lined up properly."

"Well," I said, "isn't this the tube in which Davis said he got the same results when the filament was turned off completely?"

"Oh, yes," he said, "but we always made blanks to check ourselves, with and without the voltage on."

He immediately--without giving any thought to it--he immediately had an excuse. He had a reason for not paying any attention to any wrong results. It just was built into him. He just had worked that way all along and always would. There is no question but what he is honest; he believed these things, absolutely.

Hewlett stayed there and continued to work with him for quite a while and I went in and talked it over with Davis and he was simply dumbfounded. He couldn't believe a word of it. He said, "It absolutely can't be," he said. "Look at the way we found those peaks before we knew anything about the Bohr theory. We took those values and calculated them up and they checked exactly. Later on, after we got confirmation, in order to save time, to see whether the peaks were there we would calculate ahead of time." He was so sure from the whole history of the thing that it was utterly impossible that there never had been any measurements at all that he just wouldn't believe it.

Well, he had just read a paper before the Research Laboratory at Schenectady, and he was going to read the paper the following Saturday before the National Academy of Sciences; which he did, and gave the whole paper. And he wrote me that he was going to do so on the 24th. I wrote to him on the day after I got back. Our letters crossed in the mails and he said that he had been thinking over the various things that I had told him, and his confidence wasn't shaken, so he went ahead and presented the paper before the National Academy of Sciences.

Then I wrote him a 22-page letter giving all our data and showing really that the whole approach to the thing was wrong; that he was counting hallucinations, which I find is common among people who work with scintillations if they count for too long. Barnes counted for six hours a day and it never fatigued him. Of course it didn't fatigue him, because it was all made up out of his head. (Laughter) He told us that you mustn't count the bright particles. He had a beautiful reason for why you mustn't pay any attention to the bright flashes. When Hewlett tried to check his data he said, "Why, you must be counting those bright flashes. Those things are only due to radioactive contamination or something else." He had a reason for rejecting the very essence of the thing that was important. So I wrote all this down in this letter and I got no response, no encouragement. For a long time Davis wouldn't have anything to do with it. He went to Europe for a six months leave of absence, came back later, and I took up the matter with him again.(1)

In the meantime, I sent a copy of the letter that I had written to Davis to Bohr asking him to hold it confidential but to pass it on to various people who would be trying to repeat these experiments. To Professor Sommerfeld and other people and it headed off a lot of experimental work that would have gone on. And from that time on, nobody ever made another experiment except one man in England who didn't know about the letter that I had written to Bohr.(2) And he was not able to confirm any of it. Well, a year and a half later, in 1931, there was just a short little article in the Physical Review in which they say that they haven't been able to reproduce the effect.(3) "The results reported in the earlier paper depended upon observations made by counting scintillations visually. The scintillations produced by alpha particles on a zinc sulfide screen are a threshold phenomenon. It is possible that the number of counts may be influenced by external suggestion or autosuggestion to the observer," and later in that paper they said that they had not been able to check any of the older data. And they didn't even say that the tube was gassy. (Laughter)

To me, the thing is extremely interesting, that men, perfectly honest, enthusiastic over their work, can so completely fool themselves. Now what was it about that work that made it so easy for them to do that? Well, I began thinking of other things. I had seen R. W. Wood and told him about this phenomenon because he's a good experimenter and doesn't make such mistakes himself very often, if at all. And he told me about the N-rays that he had an experience with back in 1904. So I looked up the data on the N-rays.(4, 5)

N-rays

In 1903, Blondlot, who was a well-thought-of French scientist, member of the Academy of Sciences, was experimenting with x-rays as almost everybody was in those days. The effect that he observed was something of this sort. I won't give the whole of it, I'll just give a few outstanding points. He found that if you have a hot wire, a platinum wire, or a Nernst filament or anything that's heated very hot inside an iron tube and you have a window cut in it and you have a piece of aluminum about 1/8 of an inch thick on it, that some rays come out through that aluminum window. Oh, it can be as much as two or three inches thick and go through aluminum, these rays can, but not through iron. The rays that come out of this little window fall on a faintly illuminated object, so that you can just barely see it. You must sit in a dark room for a long time and he used a calcium sulfide screen which can be illuminated with light and gave out a very faint glow which could be seen in a dark room. Or he used a source of light from a lamp shining through a pinhole and maybe through another pinhole so as to get a faint light on a white surface that was just barely visible.

Now he found that if you turn this lamp on so that these rays that come out of this little aluminum slit would fall on this piece of paper that you are looking at, you could see it much better. Oh, much better, and therefore you could tell whether the rays would go through or not. He said later that a great deal of skill is needed. He said you mustn't ever look at the source. You don't look directly at it. He said that would tire your eyes. Look away from it, and he said pretty soon you'll see it, or you don't see it, depending on whether the N-rays are shining on this piece of paper. In that way, you can detect whether or not the N-rays are acting.

Well, he found that N-rays could be stored up in things. For example, you could take a brick. He found that N-rays would go through black paper and would go through aluminum. So he took some black paper and wrapped a brick up in it and put it out in the street and let the sun shine through the black paper into the brick and then he found that the brick would store N-rays and give off the N-rays even with the black paper on it. He would bring it into the laboratory and you then hold that near the piece of paper that you're looking at, faintly illuminated, and you can see it much more accurately. Much better, if the N-rays are there, but not if it's too far away. Then, he would have very faint strips of phosphorescent paint and would let a beam of N-rays from two slits come over and he would find exactly where this thing intensified its beam.

Well, you'd think he'd make such experiments as this. To see if with ten bricks you got a stronger effect than you did with one. No, not at all. He didn't get any stronger effect. It didn't do any good to increase the intensity of the light. You had to depend upon whether you could see it or whether you couldn't see it. And there, the N-rays were very important.

Now, a little later, he found that many kinds of things gave off N-rays. A human being gave off N-rays, for example. If someone else came into the room, then you probably could see it. He also found that if someone made a loud noise that would spoil the effect. You had to be silent. Heat, however, increased the effect, radiant heat. Yet that wasn't N-rays itself. N-rays were not heat because heat wouldn't go through aluminum. Now he found a very interesting thing about it was that if you take the brick that's giving off N-rays and hold it close to your head it goes through your skull and it allows you to see the paper better. Or you can hold the brick near the paper, that's all right too.

Now he found that there were some other things that were like negative N-rays. He called them N'-rays. The effect of the N'-rays is to decrease the visibility of a faintly illuminated slit. That works too, but only if the angle of incidence is right. If you look at it tangentially you find that the thing increases the intensity when you look at it from this point of view. It decreases if you look at it normally and it increases if you look at it tangentially. All of which is very interesting. And he published many papers on it. One right after the other and other people did too, confirming Blondlot's results. And there were lots of papers published and at one time about half of them that were confirming the results of Blondlot. You see, N-rays ought to be important because x-rays were known to be important and alpha rays were, and N-rays were somewhere in beteeen so N-rays must be very important. (Laughter)

Well, R. W. Wood heard about these experiments--everybody did more or less. So R. W. Wood went over there and at that time Blondlot had a prism, quite a large prism of aluminum, with a 60° angle and he had a Nernst filament with a little slit about 2 mm wide. There were two slits, 2 mm wide each. This beam fell on the prism and was refracted and he measured the refractive index to three significant figures. He found that it wasn't monochromatic, that there were several different components to the N-rays and he found different refractive indices for each of these components. He could measure three or four different refractive indices each to two or three significant figures, and he was repeating some of these and showing how accurately they were repeatable, showing it to R. W. Wood in this dark room.

Well, after this had gone on for quite a while, and Wood found that he was checking these results very accurately, measuring the position of the little piece of paper within a tenth of a millimeter although the slits were 2 mm wide, and Wood asked him about that. He said, "How? How could you, from just the optics of the thing, with slits two millimeters wide, how do you get a beam so fine that you can detect its position within a tenth of a millimeter?"

Blondlot said, "That's one of the fascinating things about the N-rays. They don't follow the ordinary laws of science that you ordinarily think of." He said, "You have to consider these things all by themselves. They are very interesting, but you have to discover the laws that govern them."

Well, in the meantime, the room being very dark, Wood asked him to repeat some of these measurements which he was only too glad to do. But in the meantime, R. W. Wood put the prism in his pocket and the results checked perfectly with what he had before. (Laughter) Well, Wood rather cruelly published that.(6, 7) And that was the end of Blondlot.

Nobody accounts for by what methods he could reproduce those results to a tenth of a millimeter. Wood said that he seemed to be able to do it but nobody understands that. Nobody understands lots of things. But some of the Germans came out later--Pringsheim was one of them--came out with an extremely interesting story. They had tried to repeat some of Blondlot's experiments and had found this. One of the experiments was to have a very faint source of light on a screen of paper and to make sure that you are seeing the screen of paper you hold your hand up like this and move it back and forth. And if you can see your hand move back and forth then you know it is illuminated. One of the experiments that Blondlot made was that the experiment was made much better if you had some N-rays falling on the piece of paper. Pringsheim was repeating these in Germany and he found that if you didn't know where the paper was, whether it was here or here (in front or behind your hand), it worked just as well. That is, you could see your hand just as well if you held it back of the paper as if you held it in front of it. Which is the natural thing, because this is a threshold phenomenon. And a threshold phenomenon means that you don't know, you really don't know, whether you are seeing it or not. But if you have your hand there, well, of course, you see your hand because you know your hand's there, and that's just enough to win you over to where you know that you see it. But you know it just as well if the paper happens to be in front of your hand instead of in back of your hand, because you don't know where the paper is but you do know where your hand is. (Laughter)

Mitogenetic Rays

Well, let's go on. About 1923, there was a whole series of papers by Gurwitsch and others. There were hundreds of them published on mitogenetic rays.(8) There are still a few of them being published. I don't know how many of you have ever heard of mitogenetic rays. They are rays that are given off by growing plants, living things, and they were proved, according to Gurwitsch, that they were something that would go through quartz but not through glass. They seemed to be some sort of ultraviolet light.

The way they studied these was this. You had some onion roots--onions growing in the dark or in the light and the roots will grow straight down. Now if you had another onion root nearby, and this onion root was growing down through a tube or something, going straight down, and another onion root came nearby, this would develop so that there were more cells on one side than the other. One of the tests they had made at first was that this root would bend away. And as it grew this would change in direction which was evidence that something had traveled from one onion root to the other. And if you had a piece of quartz in between it would do it, but if you put glass in between it wouldn't. So this radiation would not go through glass but it would go through quartz.

Well, it started in that way. Then everything gave off mitogenetic rays, anything that remotely had anything to do with living things. And then they started to use photoelectric cells to check it and whatever they did they practically always found that if you got the conditions just right, you could just detect it and prove it. But if you looked over those photographic plates that showed this ultraviolet light you found that the amount of light was not much bigger than the natural particles of the photographic plate so that people could have different opinions as to whether it did or didn't show this effect and the result was that less than half of the people who tried to repeat these experiments got any confirmation of it; and so it went. Well, I'll go on before I get too far along.

Characteristic Symptoms of Pathological Science

The characteristics of this Davis-Barnes experiment and the N-rays and the mitogenetic rays, they have things in common. These are cases where there is no dishonesty involved but where people are tricked into false results by a lack of understanding about what human beings can do to themselves in the way of being led astray by subjective effects, wishful thinking or threshold interactions. These are examples of pathological science. These are things that attracted a great deal of attention. Usually hundreds of papers have been published upon them. Sometimes they have lasted for fifteen or twenty years and then they gradually die away.

Now, the characteristic rules are these (see Table I):

TABLE I

Symptoms of Pathological Science:

1. The maximum effect that is observed is produced by a causative agent of barely detectable intensity, and the magnitude of the effect is substantially independent of the intensity of the cause.
2. The effect is of a magnitude that remains close to the limit of detectability; or, many measurements are necessary because of the very low statistical significance of the results.
3. Claims of great accuracy.
4. Fantastic theories contrary to experience.
5. Criticisms are met by ad hoc excuses thought up on the spur of the moment.
6. Ratio of supporters to critics rises up to somewhere near 50% and then falls gradually to oblivion.

The maximum effect that is observed is produced by a causative agent of barely detectable intensity. For example, you might think that if one onion root would affect another due to ultraviolet light, you'd think that by putting on an ultraviolet source of light you could get it to work better. Oh no! OH NO! It had to be just the amount of intensity that's given off by an onion root. Ten onion roots wouldn't do any better than one and it doesn't make any difference about the distance of the source. It doesn't follow any inverse square law or anything as simple as that, and so on. In other words, the effect is independent of the intensity of the cause. That was true in the mitogenetic rays, and it was true in the N-rays. Ten bricks didn't have any more effect than one. It had to be of low intensity. We know why it had to be of low intensity: so that you could fool yourself so easily. Otherwise, it wouldn't work. Davis-Barnes worked just as well when the filament was turned off. They counted scintillations.

Another characteristic thing about them all is that, these observations are near the threshold of visibility of the eyes. Any other sense, I suppose, would work as well. Or many measurements are necessary, many measurements because of very low statistical significance of the results. In the mitogenetic rays particularly it started out by seeing something that was bent. Later on, they would take a hundred onion roots and expose them to something and they would get the average position of all of them to see whether the average had been affected a little bit by an appreciable amount. Or statistical measurements of a very small effect which by taking large numbers were thought to be significant. Now the trouble with that is this. There is a habit with most people, that when measurements of low significance are taken they find means of rejecting data. They are right at the threshold value and there are many reasons why you can discard data. Davis and Barnes were doing that right along. If things were doubtful at all why they would discard them or not discard them depending on whether or not they fit the theory. They didn't know that, but that's the way it worked out.

There are claims of great accuracy. Barnes was going to get the Rydberg constant more accurately than the spectroscopists could. Great sensitivity or great specificity, we'll come across that particularly in the Allison effect.

Fantastic theories contrary to experience. In the Bohr theory, the whole idea of an electron being captured by an alpha particle when the alpha particles aren't there just because the waves are there doesn't make a very sensible theory.

Criticisms are met by ad hoc excuses thought up on the spur of the moment. They always had an answer--always.

The ratio of the supporters to the critics rises up somewhere near 50% and then falls gradually to oblivion. The critics can't reproduce the effects. Only the supporters could do that. In the end, nothing was salvaged. Why should there be? There isn't anything there. There never was. That's characteristic of the effect. Well, I'll go quickly on to some of the other things.

Allison Effect

The Allison effect is one of the most extraordinary of all.(9) It started in 1927. There were hundreds of papers published in the American Physical Society, the Physical Review, the Journal of the American Chemical Society--hundreds of papers. Why, they discovered five or six different elements that were listed in the Discoveries of the Year. There were new elements discovered--Alabamine, Virginium, a whole series of elements and isotopes were discovered by Allison.

The effect was very simple. There is the Faraday effect by which a beam of polarized light passing through a liquid which is in a magnetic field is rotated--the plane of polarization is rotated by a longitudinal magnetic field. Now that idea has been known for a long time and it has a great deal of importance in connection with light shutters. At any rate, you can let light through or not depending upon the magnetic field. Now the experiment of Allison's was this (Fig. 3). They had a glass cell and a coil of wire around it (B_1, B_2) and you have wires coming up here, a Lecher system. Here you have a spark gap, so a flash of light comes through here and goes through a Nicol prism over here and another one over here, and you adjust this one with a liquid like water or carbon disulfide or something like that in the cell so that there was a steady light over here. If you have a beam of light and you polarize it and then you turn on a magnetic field, why you see that you could rotate the plane of polarization. There will be an increase in the brightness of the light when you put a magnetic field on here. Now they wanted to find the time delay, how long it takes. So they had a spark and the same field that produced the spark induced a current through the coil, and by sliding this wire along the trolley of the Lecher system, they could cause a compensating delay. The sensivity of this thing was so great that they could detect differences of about 3×10^{-10} seconds. By looking in here they could see these flashes of light, the light from the sparks, and they tried to decide as they changed the position of this trolley whether it got brighter or dimmer and they set it for a minimum, and measured the position of the trolley. They put in here--in this glass tube--they put a water solution and added some salt to it. And they found that the time lag was changed, so that they got a change in the time lag depending upon the presence of salts.

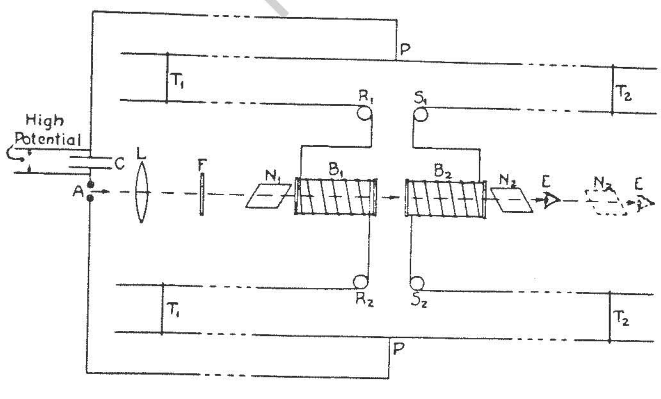

Fig. 3 Diagram of apparatus and connections. [Copy from F. Allison, Phys. Rev., 30, 66 (1927), Fig. 1].

Now they first found--very quickly--that if you put in a thing like ethyl alcohol that you got one characteristic time lag, and with acetic acid another one, quite different. But if you had ethyl acetate you got the sum of the two. You got two peaks. So that you could analyze ethyl acetate and find the acetic acid and the ethyl alcohol. Then they began to study salt solutions and they found that only the metal elements counted but they didn't act as an ion. That is, all potassium ions weren't the same, but potassium nitrate and potassium chloride and potassium sulfate all had quite characteristic different points, that were a characteristic of the compound. It was only the positive ion that counted and yet the negative ions had a modifying effect. But you couldn't detect the negative ions directly.

Now they began to see how sensitive it was. Well, they found that any intensity more than about 10^{-8} molar solution would always produce the maximum effect, and you'd think that that would be kind of discouraging from the analytical point of view, but no, not at all. And you could make quantitative measurements to about three significant figures by diluting the solutions down to a point where the effect disappeared. Apparently, it disappeared quite sharply when you got down to about 10^{-8} or 3.42×10^{-8} in concentration, or something of that sort and then the effect would disappear. Otherwise, you would get it, so that you could detect the limit within this extraordinary degree of accuracy.

Well, they found that things were entirely different, even in these very dilute solutions, in sodium nitrate from what it was with sodium chloride. Nevertheless, it was a characteristic which depended upon the compound even though the compound was disassociated into ions at those concentrations. That didn't make any difference but it was fact that was experimentally proven. They then went on to find that the isotopes all stick right out like sore thumbs with great regularity. In the case of lead, they found sixteen isotopes. These isotopes were quite regularly spaced so that you could get 16 different positions and you could assign numbers to those so that you can identify them and tell which they are. Unfortunately, you couldn't get the concentrations quantitatively, even the dilution method didn't work quite right because they weren't all equally sensitive. You could get them relatively but only approximately. Well, it became important as a means of detecting elements that hadn't yet been discovered, like Alabamine and elements that are now known, and filling out the periodic table. All the elements in the periodic table were filled out that way and published.

But a little later, in 1945 or 46, I was at the University of California. Owen Latimer who is now Head of the Chemistry Department there--not Owen Latimer, Wendell Latimer--had had a bet with G. N. Lewis (in 1932). He said, "There's something funny about this Allison effect, how they can detect isotopes." He had known somebody who had been down with Allison and who had been very much impressed by the effect and he said to Lewis, "I think I'll go down and see Allison, to Alabama, and see what there is in it. I'd like to use some of these methods."

Now people had begun to talk about spectroscopic evidence that there might be traces of hydrogen of atomic weight three. It wasn't spoken of as tritium at that time but hydrogen of atomic weight three that might exist in small amounts. There was a little spectroscopic evidence for it and Latimer said, "Well, this might be a way of finding it. I'd like to be able to find it." So he went and spent three weeks at Alabama with Allison and before he went he talked it over with G. N. Lewis about what he thought the prospects were and Lewis said, "I'll bet you ten dollars you'll find that there's nothing in it." And so they had this bet on. He went down there and he came back. He set up the apparatus and made it work so well that G. N. Lewis paid him the ten dollars. (Laughter) He then discovered tritium and he published an article in the Physical Review.(10) Just a little short note saying that using Allison's method he had detected the isotope of hydrogen of atomic weight three. And he made some sort of estimate as to its concentration.

Well, nothing more was heard about it. I saw him then, seven or eight years after that. I had written these things up before, about this Allison effect, and I told him about this point of view and how the Allison effect fits all these characteristics. Well, I know at that time at one of the meetings of the American Chemical Society there was great discussion as to whether to accept papers on the Allison effect. There they decided: No, they would not accept any more papers on the Allison effect, and I guess the Physical Review did too. At any rate, the American Chemical Society decided that they would not accept any more manuscripts on the Allison effect. However, after they had adopted that as a firm policy, they did accept one more a year or two later because here was a case where all the people in the faculty here had chosen twenty or thirty different solutions that they had made up and they had labeled them all secretly and they had taken every precaution to make sure that nobody knew what was in these solutions, and they had given them to Allison and he had used his method on them and he had gotten them all right, although many of them were at concentrations of 10^{-6} and so on, molar. That was sufficiently definite--good experimental methods--and it was accepted for publication by the American Chemical Society but that was the last.(11) You'd think that would be the beginning, not the end.

Anyway, Latimer said, "You know, I don't know what was wrong with me at that time," He said, "After I published that paper I never could repeat the experiments again. I haven't the least idea why." "But," he said, "Those results were wonderful, I showed them to G. N. Lewis and we both agreed that it was all right. They were clean cut. I checked myself every way I knew how to. I don't know what else I could have done, but later on I just couldn't ever do it again."

I don't know what it is. That's the kind of thing that happens in all of these. All the people who had anything to do with these things find that when you get through with them--you can't account for Bergen Davis saying that they didn't calculate those things from the Bohr theory, that they were found by empirical methods without any idea of the theory. Barnes made the experiments, brought them in to Davis, and Davis calculated them up and discovered all of a sudden that they fit the Bohr theory. He said Barnes didn't have anything to do with that. Well, take it or leave it, how did he do it? It's up to you to decide. I can't account for it. All I know is that there was nothing salvaged at the end, and therefore none of it was ever right, and Barnes never did see a peak. You can't have a thing halfway right.

Extrasensory Perception

Well, there's Rhine. I spent a day with Rhine at Duke University at the meeting of the American Chemical Society, probably about 1934. Rhine had published a book and I'll just tell you a few things. First of all, I went in and told Rhine these things. I told him the whole story. I said these things (Table I) are the characteristics of those things that are't so. They are all characteristics of your thing too. (Laughter) He said, "I wish you'd publish that. I'd love to have you publish it. That would stir up an awful lot of interest." He said, "I'd have more graduate students. We ought to have more graduate students. This thing is so important that we should have more people realize its importance. This should be one of the biggest departments in the university."

Well, I won't tell you the whole story with Rhine, because I talked with him all day. He uses cards which you guess at by turning over. You have extrasensory perception. You have 25 cards and you deal them out face down, or one person looks at them, and the other person on the other side of the screen looks at them and you read his mind. The other thing is for nobody to know what the cards are, in which case they are turned over without anybody looking at them. You record them and then you look them up and see if they check and that's telepathy, or clairvoyance rather. Telepathy is when you can read another person's mind.

Now a later form of the thing would be for you to decide now and write down what the cards are going to be when they are shuffled tomorrow. That works too. (Laughter)

All of these things are nice examples where the magnitude of the effect is entirely independent of magnitude of the cause. That is, the experiments worked just as well where the shuffling is to be done tomorrow as when it was done some time ago. It doesn't make any difference in the results. There is no appreciable difference between clairvoyance and telepathy. Although, if you try to think of the mechanisms of the two, it should be quite different. In order to get the cards to telegraph you all the information that's in them as to how they are arranged, and so on, when they are stacked up on top of each other and to have it given in the right sequence, it is rather difficult to think of a mechanism. On the other hand, it is conceivable that there may be some sort of mechanism in the brain that might send out some sort of unknown messages that could be picked up by some other brain. That's a different order of magnitude. A different order of difficulty. But they were all the same from Rhine's point of view.

Well, now, the little things that I have are these. There are many more I could give you. Rhine said being in quite a philosophical mood, "It's funny how the mind tries to trick you." He said, "People don't like these experiments. I've had millions of these cases where the average is about 7 out of 25." You'd expect 5 out of 25 to come right by chance and on the grand average they come out, oh, out of millions, or hundreds of millions of cases, they average around 7. Well, to get 7 out of 25 would be a common enough occurrence but if you take a large number and you get 7, well you doubt the statistics or the statistical application or, above all, what I think of and I want to give you reasons for thinking, is the rejection of a small percentage of the data.

I'll go first, before I get into what Rhine said, and say this: David Langmuir, a nephew of mine, who was in the Atomic Energy Commission, when he was with the Radio Corporation of America a few years ago, he and a group of other young men thought they would like to check up Rhine's work so they got some cards and they spent many evenings together finding how these cards turned up and they got well above 5. They began to get quite excited about it and they kept on, and they kept on, and they were right on the point of writing Rhine about the thing. And they kept on a little longer and things began to fall off, and fall off a little more, and they fell off a little more. And after many, many, many days, they fell down to an average of five--grand average--so they didn't write to Rhine. Now if Rhine had received that information, that this reputable body of men had gone ahead and gotten a value of 8 or 9 or 10 after so many trials, why he would have put it in his book. How much of that sort of thing, when you are fed information of that sort by people who are interested--how are you going to weigh the things that are published in the book?

Now an illustration of how it works is this. He told me that, "People don't like me," he said. "I took a lot of cards and sealed them up in envelopes and I put a code number on the outside, and I didn't trust anybody to know that code. Nobody! "

(A section of the speech is missing at this point. It evidently described some tests that gave scores below 5.) "... the idea of having this thing sealed up in the cards as though I didn't trust them, and therefore to spite me they made it purposely low."

"Well," I said, "that's interesting--interesting a lot, because you said that you'd published a summary of all of the data that you had. And it comes out to be 7. It is now within your power to take a larger percentage including those cards that are sealed up in those envelopes which could bring the whole thing back down to five. Would you do that?"

"Of course not," he said. "That would be dishonest."

"Why would it be dishonest?"

"The low scores are just as significant as the high ones, aren't they? They proved that there's something there just as much, and therefore it wouldn't be fair."

I said, "Are you going to count them, are you going to reverse the sign and count them, or count them as credits?"

"No, No," he said.

I said, "What have you done with them? Are they in your book?"

"No."

"Why, I thought you said that all your values were in your book. Why haven't you put those in?"

"Well," he said, "I haven't had time to work them up."

"Well, you know all the results, you told me the results."

"Well," he said, "I don't give the results out until I've had time to digest them."

I said, "How many of these things have you?" He showed me filing cabinets--a whole row of them. Maybe hundreds of thousands of cards. He has a filing cabinet that contained nothing but these things that were done in sealed up envelopes. And they were the ones that gave the average of five.

Well, we'll let it stand at that. A year or so later, he published a new volume of his book. In that, there's a chapter on the sealed up cards in the envelopes and they all come up to around seven. And nothing is said about the fact that for a long time they came down below five. You see, he knows if they come below five, he knows that isn't fair to the public to misrepresent this thing by including those things that prove just as much a positive result as though they came above. It's just a trick of the mind that these people do to try to spite you and of course it wouldn't be fair to publish.(12)

Flying Saucers

I'm not going to talk about flying saucers very much except just this. A flying saucer is not exactly science, although some scientific people have written things about them. I was a member of General Schwartz's (?) Advisory Committee after the war, and we held some very secret meetings in Washington in which there was a thing called project SIGN. I think it's s-i-g-n. Anyway, it was hushed up. It was hardly even talked about and it was the flying saucer stuff, gathering the evidence, and weighing and evaluating the data on flying saucers. And he said, "You know, it's very serious, it really looks as though there is something there." Well, I told him afterwards--I told him this story here. I said that it seems to me from what I know about flying saucers they look like this sort of thing. Well, anyway, it ended up by two men being brought to Schenectady with a boiled down group of about twenty or thirty best cases from hundreds and hundreds that they knew all about. I didn't want them all, I said to pick out about thirty or forty of the best cases, and bring them to Schenectady, and we'll spend a couple of days going over them, and he did.

Most of them were Venus seen in the evening through a murky atmosphere. Venus can be seen in the middle of the day if you know where to look for it. Almost any clear bright day especially when Venus is at its brightest, and sometimes it's caused almost panic. It has caused traffic congestion in New York City when Venus is seen in the evening near some of the buildings around Times Square and people thought it was a comet about to collide with the earth, or somebody from Mars, or something of that sort. That was a long time ago. That was thirty or forty years ago. Venus still causes flying saucers.

Well, they only had one photograph or two photographs taken by one man. It looked to me like a piece of tar paper when I first saw it and the two photographs showed the thing in entirely different shapes. I asked for more details about it. What was the weather at the time? Well, they didn't know but they'd look it up. And they got out some papers and there it was. It was taken about fifteen or twenty minutes after a violent thunderstorm out in Ohio. Well, what's more natural than some piece of tar paper picked up by a little miniature twister and being carried a few thousand feet up into the clouds and it was coming down, that's all. So what could it be? "But it was going at an enormous speed." Of course the man who saw it didn't have the vaguest idea of how far away it was. That's the trouble. If you see something that's up in the sky, a light or any kind of an object, you haven't the vaguest idea of how big it is. You can guess anything you like about the speed. You ask people how big the moon is. Some say it is as big as your fist, or as big as a baseball. Some say as big as a house. Well, how big is it really? You can't tell by looking at it. How can you tell how big a flying saucer is? Well, anyway, after I went through these things I didn't find a single one that made any sense at all. There was nothing consistent about them. They were all things that suffered from these facts. They were all subjective. They were all near a threshold. You don't know what the threshold is exactly in detecting the velocity of an object that you see up in the sky, where you don't know whether it's a thousand feet or ten thousand feet or a hundred thousand feet up. But they all fitted in with this general pattern, namely, that there doesn't seem to be any evidence that there is anything in them. And, anyway, these men were convinced and they ended project SIGN. And later the whole thing was declassified and the thing was written up by the Saturday Evening Post about four or five years ago. At any rate, that seemed to be the end of it. But, of course, the newspapers wouldn't let a thing like that die. (Laughter) It keeps coming up again, and again, and again, and the old story keeps coming back again. It always has. It's probably hundreds of years old anyway.

Well, I think that's about all. If there are any questions, I'd be happy to say more.

Question Period

(W. C. White):

People may want to go now because it's quarter after five though I'm sure Dr. Langmuir would be glad to discuss this some more.

I was going to add another one to these characteristics. Isn't the desire for publicity another of the characteristics?

A. Well, it is in Rhine's case. There is no question about that. Rhine, I think, ... thinks he's honest, but I know perfectly well that he--everything he says, he talks about the importance of getting more students, and the importance of having the people in his own university understand the importance of this thing and so on. And then the fact that no man in his senses could discard data the way he did those things sealed up in the cards. So I don't hold a very high value on his work. Now the other people, I don't have the slightest doubt but what these men are really honest. They are sincere. They loved publicity; Allison, of course, loved to publish about new elements one after the other. These were published by the American Chemical Society; and Latimer liked to publish his little article on tritium, the first discovery of tritium. So I think that has something to do with it, but I don't think that that's the driving force. I think the driving force is quite a normal scientific desire to make discoveries and to understand things. Davis and Barnes were finding things and it was wonderful while it lasted.

Q. (Liebhafsky): I just wanted to point out that perhaps the neatest comment on item four was made at the University of California when this business was discussed at the Research Conference there in about 1930 or 32. Professor Birge said that this effect was just Allison wonderland. (Laughter) (Langmuir): Did you ever hear Latimer talk about it?

(Liebhafsky): Well, Latimer was pushing it and you've got to allow for Latimer's persuasiveness. There were people on the faculty that I'm sure never believed it.

(Langmuir): But it was funny that G. N. Lewis would believe it.

(Liebhafsky): Well, you know that there is a very close personal relationship between Latimer and Lewis.

(Langmuir): I understand that Lewis got back his ten dollars. (Laughter)

Q. How would an analysis like this apply to religious experiences?

A. Well, the method of approach to religious questions--a lot of people think you don't want to have any evidence, you want faith; and if that's your attitude why I don't think this thing applies. But if some religious performer of a certain belief tries to argue with me, my reactions would be very much like this.

Q. In setting up these criteria, you may in a way limit the possibilities of scientific investigation. It occurred to me that suppose something happened in the heavens--some astronomical event--that nobody had ever seen before. Something that happens once in a million years. Really, I mean, supposing that you could tell. It would fit the same criterion, wouldn't it?

A. No, I don't want to depend on any one of these. I've been reading the life of Pasteur. Pasteur had the idea of germs. Everybody thought that he was a fool--thought there couldn't be any sense to the subject. It took a long time before germs were believed. People believed in spontaneous generation of new forms of life. They happened spontaneously not by the introduction of spores from the outside but spontaneously --and Pasteur had to fight that. The test of time is the thing that ultimately checks this thing. In the end, something is salvaged. You can't do that while the thing is growing, while the thing is being discussed, but in the end you do know that the Allison effect is gone. It never would be anything. And that's what I mean about these other things. We've waited long enough now. This whole pattern of things fits together with the idea that you're at a threshold. You're right at the point where things are very difficult to see--that's what I want to bring out. Now, in Pasteur's experiments, when he killed anthrax in animals, he got 25 right out of 25. The sheep all died or they didn't die. There was no threshold value about it. People who didn't know anything about it might have thought so, but when they saw one experiment they were convinced.

One more question -

Q. These criteria that you put down would apply very well to the theory of relativity with measurements of very small fractions of a degree of arc in the neighborhood of a bright disk of the sun.

A. Yes, well now take an example I've often thought of. There are lots of scientific instances. They go through the same sort of stage. For instance, in Laue and Bragg's theory of x-rays being electromagnetic waves. When the first reports came out you had to keep an absolutely open mind about them. You didn't know but what this was just another case of wishful thinking. But how long did it take? Within three or four years they were making precision measurements of the wavelengths of x-rays--very, very few years. Now, that's just what doesn't happen in these things. So you have to wait a little time for these things to prove themselves but I don't think that you will find that there's anything more than a superficial resemblance. Take the first experiments of the wave theory of electrons. The first evidence was very poor, and more people had to be brought in, but to me the important thing was not how it looked at the time but the quickness with which those results were resolved as contrasted to these things that hang fire and hang fire. Now the Davis-Barnes effect and the N-rays were quenched suddenly; but most of these other things go on, and on, and on, and on.

(White): I believe that this is the latest lasting colloquium we've ever had that I remember. It was a great privilege to have such a speaker. We thank you, Dr. Langmuir.

EPILOGUE (R.N. Hall)

Pathological science is by no means a thing of the past. In fact, a number of examples can be found among current literature, and it is reasonable to suppose that the incidence of this kind of "science" will increase at least linearly with the increase in scientific activity.

Professor Allison has retired, but in a recent letter he wrote that his investigations of the Allison Effect have suffered long interruptions but were never abandoned, and he spends summers and occasional weekends working on it with students at Auburn University. The effect is also being investigated under a contract with the Air Force Aero Propulsion Laboratory at the University of Dayton.(9e)

Flying Saucers are still very much with us. As Langmuir said, "Of course, the newspapers wouldn't let a thing like that die." How right he was!

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A few neutrons each second (or a few thousand) from an electrolytic cell may be cold nuclear fusion or may have an 'arcs and sparks' origin. Within the next few weeks, experiments will surely show whether cold nuclear fusion is taking place; if so, it will teach us much besides humility and may indeed provide insight into significant geophysical puzzles[3]. Large heat release from fusion at room temperature[1] would be a multi-dimensional revolution. I bet against its confirmation.

Richard L. Garwin

Richard L. Garwin is a research fellow with IBM at the T.J.Watson Research Center, Yorktown Heights, NY, and adjunct professor of physics at Columbia University.

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Somebody is going to have to eat his hat (L. Maiani).

We are also human, and need miracles, and hope they exist. (L. Ponomarev, Moscow).

A few neutrons each second (or a few thousand) from an electrolytic cell may be cold nuclear fusion or may have an 'arcs and sparks' origin. Within the next few weeks, experiments will surely show whether cold nuclear fusion is taking place; if so, it will teach us much besides humility and may indeed provide insight into significant geophysical puzzles[2]. Large heat release from fusion at room temperature[1] would be a multi-dimensional revolution. I bet against its confirmation.

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